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Richard Steiner

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ON THE ORIGIN OF THE ḤéDeR ~ ḤăDáR ALTERNATION IN HEBREW [1]

by

Richard Steiner
Yeshiva University

The *ḥéðɛʀ ~ ḥăðáʀ* alternation is limited to nouns with final resonant, because resonants are particularly prone to acquisition of syllabicity, which, in turn, often leads to epenthesis. The construct forms of such nouns underwent epenthesis earlier than their absolute co-allomorphs — early enough, in fact, to be affected by the well-known Hebrew stress-shift — because they lost their case-endings earlier. The original epenthetic vowel was lowered to *a* by Philippi's law. The retention of *e* in *ḥăðél* is due to overlapping of Philippi's law and stress-shift.

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1. THE *ḤéḏeR* ~ *ḤăḏÁR* SUBCLASS OF SEGOLATE NOUNS

Scholars have long been intrigued by a small group of exceptions to one of the fundamental rules of Hebrew morphology, viz., the rule which states that the construct-state allomorph of segolate nouns is identical to the absolute-state allomorph. Both the rule and the exceptions were discussed already in the 11th century by Yonah ibn Janāḥ (1886:205-6, my translation):

"Know that that which is of the form CéCeC, with six points [∴ ∵] or with five [∴ ⁖], usually does not change when put in construct with a non-pronominal substantive, for example, *ʾéReṣ miṣRayim*, *déReX yam ṣuɣ*, *séɣeR hat-toRo(h)* ... — most of this class follows in the same path. But some of them do change when put in the construct, for example, *ḥéḏeR* in *u-va-ḥăḏáR miškovóxo*; *śéɣeR* in *śóɣáR ʾáloɣexo* although it is unchanged in *wó-xoł peṭeR śéɣeR bóhemo(h)*; *zéRaᶜ* in *ki-zRáᶜ gaó hu(ʾ)* although it is unchanged in *kó-zéRaᶜ gaó łovon*; *néṭaᶜ* (as in *wó-ᶜoóo(h) qoṣiR kó-mo néṭaᶜ*) in *nóṭáᶜ śaᶜăšuᶜow*; [*ḥéseR*] ([as in] *bó-ḥéseR u-vó-xoɣon*) in *wɛ-ʾéwiłim ba-ḥăsáR łɛv yamuθu*; *yéReq* (as in *wó-ło(ʾ) noθaR koł yéReq bo-ᶜeṣ*) in *wi-(y)Ráq déśɛ(ʾ)*. . . But it is also possible that *wi-(y)Ráq déśɛ(ʾ)* is the construct of *ṭov ʾăRuḥaθ yoRóq* ... although in that case there would not be any evidence for us in it, since it would not be a member of the *ʾéReṣ* class. And I wonder at Abu Zakariyya [Ḥayyuj] when he says that none of this class change except *ḥévɛł* in *ḥăvéł ḥăvołim* . . ."[2]

If we eliminate the one uncertain example here (*yéReq*), we are left with a group of nouns all of which end in a liquid or ᶜ:

ḥéḏeR	~	*ḥăḏáR*	'room'
śéɣeR	~	*śóɣáR*	'offspring'
zéRaᶜ	~	*zóRáᶜ*	'seed'
néṭaᶜ	~	*nóṭáᶜ*	'plant(ation)'
ḥéseR	~	*ḥăsáR*	'want, poverty'
ḥévɛł	~	*ḥăvéł*	'vanity'

Three of the new examples discovered by Ewald (1835:250, 1855:472) fit in perfectly here,

śáḥaR	~	*śóḥáR*	'trade, profits'
śévaᶜ	~	*śóváᶜ*	'seven'
téśaᶜ	~	*tóśáᶜ*	'nine'

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[2]Wa-ᶜlam ʾanna-hu mā kāna ᶜalā mitāli peᶜel bi-sittati nuqatin ʾay [l.c. Ibn Tibbon ʾaw] bi-xamsatin fa-ʾinna ʾaktara-hā lā yataġayyaru ᶜinda ʾidāfati-hā ʾilā l-ʾasmāʾi ẕ-ẕāhirati mitla ʾereṣ miṣrayim dɛrɛx yam suf sefɛr hat-toro(h) ... ᶜalā hādā yaṭṭaridu ʾaktaru l-bāb. Wa-qad yataġayyaru baᶜdu-hu ᶜinda l-ʾidāfati ka-taġayyuri ḥeᶄer fī qawli-hi u-va-ḥăḏar miškovóxo wa-ka-taġayyuri šeᶌer fī šóᶌar ʾáłofɛxo wa-ʾin kāna ġayra mutaġayyirin fī qawli-hi wó-xoł peṭɛr śéɣɛr bóhemo(h) wa-ka-taġayyuri zeraᶜ fī qawli-hi ki-zraᶜ ġaó hu(ʾ) wa-ʾin kāna ġayra mutaġayyirin fī qawli-hi kó-zeraᶜ ġaó łovon wa-ka-taġayyuri nɛṭaᶜ ʾaᶜnī wó-ᶜoóo(h) qoṣir kó-mo noṭaᶜ fī qawli-hi nóṭaᶜ šaᶜasuᶜow wa-ka-taġayyuri bó-ḥesɛr u-vó-xoɣon fī qawli-hi wɛ-ʾéwiłim ba-ḥasar lev yamuθu. Wa-ka-taġayyuri wóło(ʾ) noθar koł yɛreq bo-ᶜeṣ fī qawli-hi wi-(y)raq déśɛ(ʾ) ... Wa-rubba-mā kāna wi-(y)raq déśɛ(ʾ) muḍāfa ṭov ʾăruḥaθ yoroq ... ʾillā ʾanna-hu laysa yakūnu hīna-'iḏin lanā fī-hi šahādatan ʾiḏ laysa min bābi ʾereṣ. Wa-ʾinnī la-ʾaᶜjabu min ʾabī Zakariyyā fī qawli-hi ʾanna-hu lam yataġayyar min hāḏa l-bābi ᶜinda l-ʾidāfati ġayra ḥăvel ḥăvolim.

but Ewald's fourth new example

    qáḥaθ   ~   *qɔ̆ḥáθ*   'taking, to take'

does not. It should be grouped instead with medial pharyngal forms like:

    bắ^caḏ̆   ~   *bɔ̆^cáḏ̆*   'behind'

    **?mó^caṭ*[3]   ~   *mɔ̆^cóṭ*   'fewness'

An additional example was discovered not long ago by Berggrün (1950:7) in the Kaufmann manuscript of the Mishnah — he could have cited Codex Parma A (= De Rossi 138), as well — and Elijah Levita's *Sẽ́fɛr Ha-Tĩšbĩ* (s.v.):

    sẽ́ɣɛn   ~   *sɔ̆ɣán*   'superior priest'

It should be noted that, although neither of these singular forms occurs in the Bible, the plural form does, and it is therefore likely that the alternation attested in the Mishnah is much older than that source. This example ends in a nasal rather than a liquid, but since the liquids and nasals form a natural class (commonly called resonants), it fits in well with the other examples.

Three other construct forms deserve mention here, although it is not certain that they are actually derived from segolates: *gɔ̆ḏ̆óḷ* (Exodus 15:16), *gɔ̆vóah* (I Samuel 16:7), and *qɔ̆ḏ̆óš* (Psalms 46:5, 65:5). The idea that *gɔ̆ḏ̆óḷ* and *qɔ̆ḏ̆óš* are in some way equivalent to *góḏ̆ɛḷ* and *qóḏ̆ɛš* (the Hexapla actually READS κοδσ in Psalms 46:5) goes back at least as far as Samuel ben Meir (Exodus 15:16), but the suggestion that *gɔ̆ḏ̆óḷ*, *gɔ̆vóah*, and *qɔ̆ḏ̆óš* have something in common with *ḥắḏ̆ár*, *hắvéḷ*, etc. seems to be original with S.D. Luzzatto ([1860] 1970:7-8, [1871] 1965:290; cf. also Lambert 1931:108, Kogut 1969:23-4, Blau 1971:318). There is, however, another school of thought which maintains that *gɔ̆ḏ̆óḷ*, *gɔ̆vóah*, and *qɔ̆ḏ̆óš* are the construct forms of the adjectives *gɔvóḷ*, *gɔvóah*,[4] and *qɔḏóš* (Brockelmann 1908:II,48, Rabin 1967:7, Koehler-Baumgartner 1967, s.v. *qɔvóah*). Brockelmann (op. cit.) adduces these forms as evidence that adjectives can serve as abstract nouns in Hebrew, a proposition which becomes much more attractive when limited to adjectives in the construct state (cf. also *mar* in I Samuel 15:32, Psalms 38:15, *yɔ̆ḟa0* in Deuteronomy 21:11, and perhaps *yɔ̆ḟe(h)*[5] in I Samuel 16:12, 17:42). This syntactic explanation obviously implies that *gɔ̆ḏ̆óḷ*, *gɔ̆vóah*, and *qɔ̆ḏ̆óš* have nothing to do with *ḥắḏ̆ár*, *hắvéḷ*, etc., and we shall therefore take the prudent (and convenient) course of omitting these forms from the discussion which follows.

One last alternation which may belong here is the alternation of the infinitive construct, *qɔ̆ṭóḷ*, with its co-allomorph (before suffixes), *qɔṭḷ-*, but since the original shape of the

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[3]This form is reconstructed on the analogy of forms like *kóvɛḏ̆* 'heaviness', *ḥózɛq* 'strongness', etc. The reconstruction depends on the assumption that *mɔ̆^coṭ* is an abstract noun rather than an infinitive construct, a distinction which is perhaps dubious from a historical point of view but seems to have synchronic validity.

[4]One problem for this theory might be the fact that we expect *gɔ̆váh* as the construct of *gɔvóah* (on the analogy of alternations like *mizbéah* (abs.) ~ *mizbaḥ* (const.)), and this form actually occurs several times in the Bible. On the other hand, the retention of *o* in *gɔvóah* would seem to be just as much of a problem for those who take it as the construct of *gɔvah*.

[5]The appropriateness of this example hinges on the meaning of ^c*im* in the two verses cited. It is generally taken to mean 'and' in these verses, but it might, like English 'with', have the meaning 'possessing (a quality)'. This meaning would fit nicely in I Samuel 25:25 and Psalms 89:14 as well.

latter is uncertain due to fluctuation in the Massoretic pointing (between forms like šɔxbɔ̆xɔ and ʕɔ̆ɓi, which point to an original *CuCC, and forms like šɔxvɔ and kɔθvɔ, which point to an original *CuCuC), we cannot be sure. Moreover, if the vocalization of qɔ̆tol owes anything to epenthesis, it owes at least as much to contamination of the infinitive construct with the infinitive absolute (Joüon [1923] 1965:109-10) and the imperfect (cf. Bauer-Leander [1922] 1965:316-7). The original form of the alternation and its conditioning may accordingly be beyond recovery.

A form which does not belong here, contrary to what I once thought, is σφαρθελλειμ, the rendering of sɣr tlym "Book of Psalms" in Origen's list of the books of the Bible, reproduced by Eusebius in the *Ecclesiastical History* (Schwartz 1908:574). The form σφαρ is open to several interpretations. It could be a scribal error for *σαφρ which crept in before the time of Eusebius (σφαρ is definitely the form which Eusebius had; cf. the critical apparatus, loc. cit.), but *σαφρ would be anomalous in its own right as a rendering of Hebrew *sɣr (one would expect *σεφρ[6]).

A second possibility would be to take σφαρ as a rendering of the Aramaic construct form[7] sɣar. It is true that θελλειμ has the HEBREW plural suffix and that all of the other names[8] on Origen's list are Hebrew (e.g. αμμεσφεκοδειμ[9], δαβρηϊαμειν[10]), but, as Kutscher has shown (1959:15-6,46-7), none of this is incompatible with an Aramaizing vocalization. Indeed, Origen's vocalization of the Biblical text itself is not free of Aramaisms (loc. cit.), so there is no reason to exclude the possibility of an Aramaism in his vocalization of the Bib- lical book-titles. On the other hand, it should be noted that, out of the many segolates in the extant fragments of Origen's *Hexapla* (Mercati 1958), there is not one on the pattern of σφαρ (Brønno 1943:136). Even gbr in Psalms 18:26, vocalized gɔ̆vār by the Massoretes, is vocalized γαβρ by Origen (loc. cit.). In any case, it is obvious that both of the above in- terpretations obviate the need to deal with σφαρ within the framework of this article.

The third, and most likely, hypothesis is that σφαρ is to be connected with the Samaritan Hebrew form asɣar 'book' (the initial vowel is prothetic and probably late). This form is not a segolate since it occurs (Ben Hayyim 1961) in the absolute state (e.g. Deuteronomy 31: 24,26) as well as in the construct (e.g. Deuteronomy 24:1,3). It is, rather, a noun on the

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[6]Cf. forms like ζεχρ (= Massoretic zixr-) and ρεσθ (= Massoretic rišt-) in the second column of Origen's *Hexapla* (Mercati 1958). Other forms with ε correspond to Massoretic a-stems: νεφσι (= Massoretic nafši), δερχω (= Massoretic darko).

[7]The construct still existed at this period (Kaddari 1969:104) even though d-periphrasis had become, in Kaddari's words, "the regular way of nominal subordination" (ibid., 102).

[8]It should be noted, however, that many of these titles "consist of either (1) the first word or words of the book ... or (2) the name of the hero or supposed author..." (Swete [1914] 1968:214), and that such titles cannot be adduced as evidence concerning σφαρθελλειμ which is "a description of the contents" (loc. cit.). Consequently, only titles which fall into this latter category are adduced here.

[9]This is close to the Mishnaic title of the Book of Numbers, but the vocalization differs. Codex Kaufmann has ḥummaš hap-piqqudim in *Soṭah* VII,7 and in *Menaḥot* IV,3 and ḥumaš hap- pɔqudim (the ketiv is hpyqwdym) in *Yoma* VII,1. Codex Parma A (De Rossi 138) has ḥomeš hap-piqqudim in the Yoma passage.

[10]Hebrew davre (= Tiberian divre) yamin "Chronicles" (lit. 'things of days'). Note that this title cannot be Aramaic because the Aramaic plural of yom 'day' is yomin with an o, and because the Aramaic word for 'things of' is mille. The plural suffix -in is, of course, almost as common in Mishnaic Hebrew as it is in Aramaic.

qə̆tɔ̆l pattern, virtually identical to the Late Biblical (II Chronicles 2:16)[11] and Mishnaic (*Nazir* VII.3, *Kelim* I.1, *Zavim* V.10) word sɔ̆fɔ̆r meaning 'counting'. It follows that, even according to this interpretation, σφαρ has little in common with the Hebrew segolate construct forms which are the subject of this article.[12]

2. FOUR QUESTIONS ABOUT THE ORIGIN
OF THE *ḤÉDER ~ ḤĂDÁR* ALTERNATION

We are left, then, with a highly coherent set of segolate construct forms, all of which end in a resonant or ʿ (and it is not unreasonable to theorize, as does Levin (1966:4), that the latter was also a resonant, i.e. an *a*-vowel with pharyngeal constriction but no audible friction, in ancient Hebrew, at least in the environment of C #) and are stressed on their epenthetic vowel. It is obvious that any attempt to explain how these forms originated will have to account for these facts by answering the following questions:

1) Why did the stress shift, in these forms, to the epenthetic vowel?

2) Why didn't this change take place in the absolute state of *ḥădār*, *ḥăvél*, etc. as well as in the construct?

3) Why was the change favored by a final resonant?

One final question is needed to round off this list:

4) Why does the epenthetic ε of the absolute alternate with other vowels in the construct, viz.

   a) *e* in *ḥăvél*

   b) *a* in *ḥădār*, *sɔ̆ḥār*, etc.

3. PREVIOUS ATTEMPTS TO EXPLAIN THE *ḤÉDER~ḤĂDÁR* ALTERNATION

3.1. Sievers' Theory

Previous attempts to explain the *ḥéder ~ ḥădār* alternation have tended to focus on only two or, at most, three of these questions, question 3 being the one most consistently ignored. Sievers, for example, gives the following explanation (1901b:279) for the alternation:

> "Beim Status constr. ist ja auch der Accentwechsel ganz verständlich. Je
> stärker der Status constr. enttont wird, um so mehr verliert er seine
> eigene (d.h. historisch berechtigte) Tonsilbe und ordnet sich lediglich
> dem allgemeinen rhythmischen Gefüge unter, in dem er auftritt . . ."

In other words, the weakened accent of a noun in the construct state was more easily influenced by external rhythmic factors, e.g., the accent of the following noun, requirements

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[11] I owe this reference to Professor Ben Hayyim. In fact, it was he who first called my attention to the form sɔ̆fɔ̆r and to the possibility of linking it with σφαρ.

[12] The form σφερ(τελειμ/τελιμ) which occurs in Epiphanius' list of the Biblical books (Audet [1950]1974:55) and in the genealogically related list published by Audet (ibid., 53) is, of course, even further removed from the forms treated in this article. It is also much more difficult to interpret.

of meter (in poetry), etc. This theory would lead one to expect that proper nouns, which occur only rarely (if ever) in the construct state, would be immune to shift of stress from etymological vowel to epenthetic vowel, and the fact that this is not the case (see §4.2 below) casts a heavy cloud of suspicion over the entire theory. Furthermore, it is apparent that Sievers' theory does not address itself to questions 3 and 4 at all, a defect pointed out, in part, already by Bauer and Leander ([1922]1965:574):

"Die Umfärbung des Ultimavokals bleibt hierbei ... unerklärt."

3.2. Bauer-Leander's Theory

Bauer and Leander's own explanation (loc. cit.) is designed to provide a partial remedy for the above-mentioned defect:

"Wahrscheinlich sind sie einfach nach Analogie der 4. Klasse entstanden: $d^eb\bar{a}r\acute{\imath}m : d^eb\grave{a}r = g^eb\bar{a}r\acute{\imath}m : x; \quad x = g^eb\grave{a}r$."

The same type of analogy would presumably explain the Umfärbung of h̥ăɔ̆ăr, h̥ăɪ̆ăr, s̆ɔ̆ɣăr, s̆ɔ̆ɦăr, and s̆ɔ̆ɣăn, so even if we do not agree with Bauer and Leander (ibid., 573) that gɔvăr (Psalms 18:26) is a construct form (see fn.31 below), we are still obliged to consider whether their explanation is capable of accounting for the other forms. Consider, then, the analogy which supposedly produced the singular construct form h̥ăɔ̆ăr. Should not the same analogy have produced a plural construct form *h̥ɛɔ̆ăɛ (< *h̥iɔ̆ăɛ on the analogy of divɾ̆ɛ),[13] a singular absolute form *h̥ɔɔ̆ăr (on the analogy of dɔvɔ̆r), and a suffixed singular form *h̥ăɔ̆ɾɔ̆ (on the analogy of dɔvɔ̆rɔ̆), especially since the plural construct and the singular absolute are semantically closer to the plural absolute (which stands to the left of the equals sign in the proportion) than is the singular construct.[14]

This explanation, then, leaves us in a worse position vis à vis question 2 than we were to start with: not only does it fail to answer the question, it actually strengthens it. In addition, it fails to account for the special role played by final resonants in the shift (question 3), or the ε ~ e alternation (question 4a).

3.3. Malone's Answers to Questions 1 and 4a

A third attempt to solve the riddle of the h̥ɛ̆ɔ̆ɛɾ ~ h̥ăɔ̆ăr alternation was recently made by Malone (1971). Malone answers question 1 (ibid., 53-4) by assuming that epenthesis in the construct forms under discussion (unlike epenthesis in the corresponding absolute forms) took place early enough to feed the general stress-shift which Semitists have long posited for ancient Hebrew (Bergsträsser 1918:114-5, Bauer and Leander [1922]1965:177ff, Cantineau 1931:97, Harris 1939:50, Goetze 1939, Blau 1972:81). Descriptions of this stress-shift vary from scholar to scholar, but all agree that nouns with two etymologically short stem-vowels (e.g. kɔvɛ̆ɔ̆, dɔvɔ̆r, ʕɛnɔv) were originally stressed on the first of these vowels, and that the position of the accent in Massoretic Hebrew (on the second stem-vowel) is a product of the stress-shift (Bergsträsser 1918:114, Bauer and Leander [1922]1965:178, Cantineau 1931:97,

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[13]It must be admitted, however, that it is possible to claim that Tiberian divɾ̆ɛ comes from *davɾ̆ɛ; see above, fn. 10.

[14]One might also ask why analogies of this sort did not change nouns of the form CeCɔ̆C (plural CɔCɔC-im) to CɔCɔ̆C.

Goetze 1939:442, Blau 1972:81).[15] A segolate noun which underwent epenthesis before this stress-shift would clearly have been part of this larger class of nouns, and would, there-fore, have participated in the stress-shift along with the other members of this class.[16] The same conclusion was reached independently by the author of these lines.[17]

Malone's answer to question 4a is also new. He assumes (op. cit., 46) that the original epenthetic vowel was *e*, and that this *e* was later lowered to ɛ in unstressed position. Malone does not motivate this solution, apparently expecting his readers to realize that the latter assumption is necessary in any case to account for such alternations as: *šém ~ šɛm-, tɔšém ~ ʾal-tɔšɛm, haggéð ~ haggɛð-nɔ́, yešév ~ yešɛv-nɔ́.*

Malone's answers to questions 1 and 4a are very appealing for the simple reason that they require very few assumptions beyond those required to account for other Hebrew phenomena. In dealing with questions 2 and 4b, on the other hand, Malone (op. cit., 58) offers no solutions beyond those set forth by Sievers (question 2) and Bauer and Leander (question 4b), which, as we have seen, are far from economical. The answers which we are about to give to these questions — and to question 3 — are, the author believes, more in harmony with the multiply motivated solutions to questions 1 and 4a which Malone put forth.

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[15]Most of these scholars believe that the stress-assignment rule of Classical Arabic (the so-called *Dreisilbengesetz*) originally held for Hebrew as well. This rule assigns the stress in this word-class to the first stem-vowel. Goetze agrees with this result for his own reasons. His position (1939:442-3) is that the first stem-vowel would have been syncopated if it had not been stressed — and the contrast between the treatment of short *i* in *zɔ́rõaʿ < *ðirãʿ, hǎmõr < *himãr, and tɔ́hõl < *tihãl,* on the one hand, and the treat-ment of that same vowel in *ʿenɔ́v < *ʿʿinab and șelɔ́ʿ < *dilaʿ,* on the other (ibid., 444) would certainly seem to bear him out. The fact that the Akkadian reflexes of this class are also stressed on the first syllable is further proof, according to Goetze (ibid., 444-5). This latter argument is developed and refined in Steiner 1975 (cf. esp. p. 8).

[16]That early epenthesis (in the vicinity of a final resonant!) shows up as a shift of form-class in Tigre as well is clear from Gragg's description (1974:3):

> "Te does not tolerate final clusters either, but uses various processes to get rid of them. The most frequent is epenthesis (φ > ə/C_C# : *kalb > kaləb*) ...; but shift in form class (*báql > bäqäl,* so frequently for CäCR, where R is a resonant) and deletion of homorganic consonants (*qärn > qär*) are also attested."

The synchronic repercussion of early SYNCOPE is also a shift of form-class, cf. *kɔvéð ~ kévɛð, kɔθéṣ ~ kéθɛṣ, yɔréx ~ yérɛx, gɔðér ~ géðɛr, ʿɔrél ~ ʿérɛl, șelɔ́ʿ ~ šélaʿ, šeʿɔ́r ~ šáʿar, ʾɔšɔ́n ~ ʿéšɛn, šɔxɔ́r ~ šéxɛr, ʾɔrɔ́x ~ ʾérɛx, ʾɔšéð ~ ʾéšɛð* (Steiner 1975:3-4).

[17]As Malone points out (1971:60fn58), this same rule-ordering (or "change-ordering") device was employed by Bauer and Leander ([1922]1965:213) to explain the shift of the accent in other classes of nouns (cf. also Goetze 1939:447). It is not entirely clear why these scholars did not extend this solution to the class of construct forms under discussion. It is possible that they were put off by the fact that it leaves the *Umfärbung* of the epenthetic vowel (in forms like *hǎdár,* etc.) unexplained. But then we may legitimately ask why they didn't use the analogy theory IN CONJUNCTION WITH the rule-ordering theory as does Malone. By doing so they would have strengthened their analogy theory, since the probability of any given analogy taking place would appear to rise in direct proportion to the similarity between the analogandum and the analogans. This consideration clearly outweighs the "economy" of allowing one theory (viz. the analogy theory) to account for both *Umfärbung* and stress-shift.

4. NEW ANSWERS TO QUESTIONS 2, 3, AND 4b

4.1. Question 2 (The Stress Shift)

It has often been noted that alternations like zɔqḗn ~ zə̆qā́n, internally reconstructed to obey Philippi's law (which is, in turn, based on an internally reconstructed version of zɔqḗn ~ zɔqā́ntī, and states that short, stressed, checked, non-low, front vowels become low) prove that final vowels (case endings) were apocopated earlier in the construct state than in the absolute (Bergsträsser 1918:115,149, Bauer and Leander [1922]1965:523, Harris 1939:41, Blau 1972:69,223).

CONSTRUCT APOCOPE

| FEEDS

PHILIPPI'S LAW

FED BY | COUNTER

ABSOLUTE APOCOPE
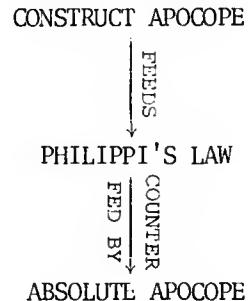
Figure 1

Since the loss of case endings is a necessary pre-condition for epenthesis, it seems reasonable to assume that the construct state was the leader in the area of epenthesis as well:

CONSTRUCT APOCOPE

FEEDS   FEEDS

PHILIPPI'S LAW   CONSTRUCT EPENTHESIS

FED BY | COUNTER

ABSOLUTE APOCOPE

FEEDS

ABSOLUTE EPENTHESIS

Figure 2

If so, we may answer question 2 simply by assuming that stress-shift occurred after nouns in the construct state underwent epenthesis but before nouns in the absolute state did so:

CONSTRUCT APOCOPE
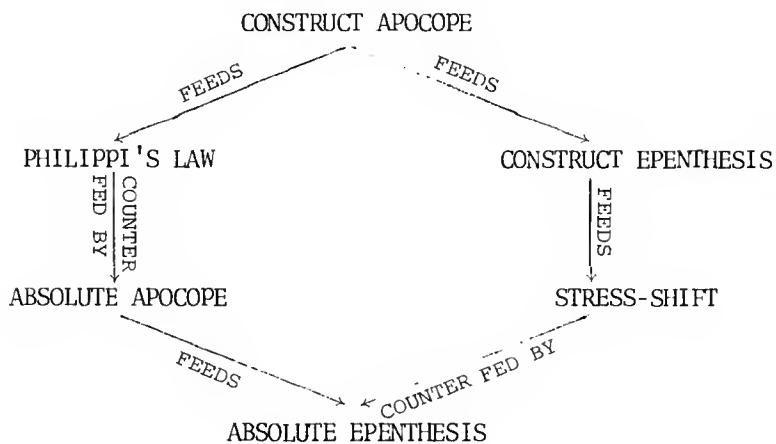
Figure 3

The claim is, then, that the stress did not shift to the epenthetic vowel in forms like *ḥéðɛr, ḥévɛl*, etc. because these forms did not get their epenthetic vowels until after the extinction of stress-shift.

It should not surprise us to learn that epenthesis proceeded in stages. The two stages we have identified coincide more or less with the stages discovered by Bauer and Leander (op. cit., 213). The existence of a third stage has been tentatively proposed (Harris 1941:145), based on the contrast between the behavior of the *bgd kpt* stops following an epenthetic vowel in a noun and the behavior of those consonants following an epenthetic vowel in a verb (cf. especially the minimal pair *ləqáḥaθ* 'to take'[18] : *ləqáḥat*[19] 'you (f.) took'). This contrast would seem to prove that epenthesis took place later in the verb,[20] too late to be

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[18]In Hebrew, infinitives pattern with the nouns because they are derived from verbal nouns.

[19]The unspirantized *t* in this form is a regular feature of the 2fs perfect suffix, cf. *šɔxáḥat* 'you forgot', *šɔláḥat* 'you sent', *poθáḥat* 'you opened', *yɔðáᶜat* 'you knew', *yɔyáᶜat* 'you toiled', *šɔmáᶜat* 'you heard', *higgáᶜat* 'you arrived', *hišbáᶜat* 'you satiated', *pošáᶜat* 'you transgressed'.

[20]Just as the early date of construct epenthesis is a function of the early date of construct apocope, so too the late date of epenthesis in *wayyíhad* (note, in addition to the unspirantized *d*, the unlengthened, or at least unlowered, *i* of this form and others like it in contrast with the *e* of *yeḥám*) and *ləqáḥat, šɔxáḥat*, etc. may be a function of the late date of apocope in final-*w,y* verbs and in the second person feminine singular of the perfect. Evidence for the late date of apocope in the latter comes from the un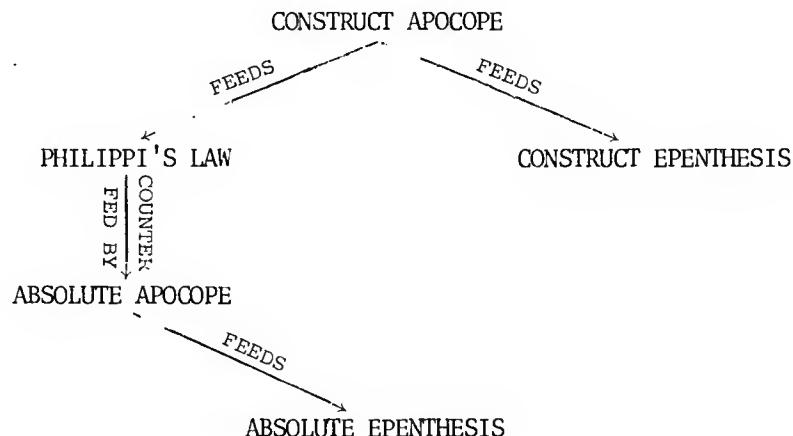spirantized (and simplex?) *t* of *noθat(t?)* 'you gave', in which apocope took place too late for the final *t* to be affected by spirantization (contrast *mattóθ* 'gift'), either because spirantization was already extinct or because geminate simplification, a prerequisite for spirantization, was already extinct. This theory is not necessarily in conflict with Blau's theory that apocope took place EARLIER in the verb than in the absolute-state noun (1972: 65), because that theory was put forward to account for forms like *šomár* and *yixbáð* in which we find *a* instead of *ɔ* in syllables which were originally open. There is no good reason for assuming that apocope in *wayyíhad* and *ləqaḥat* was as early as apocope in *šomár*

affected by spirantization.[21] If so, we obtain a tri-partite division:

CONSTRUCT EPENTHESIS   qɔ̆ḥāθ

--STRESS-SHIFT

ABSOLUTE EPENTHESIS   (lɔ)qā́ḥaθ

--SPIRANTIZATION

VERB EPENTHESIS   lɔqā́ḥat

Figure 4

It is sobering to realize that, were it not for the fact that two of the sound changes which took place while epenthesis was in progress (viz. stress-shift and spirantization) happened to be fed by it, the Massoretic reflex of *qaḥt would have been identical in the construct, in the absolute, and in the verb, and we might never have suspected that epenthesis cannot be assigned to only one position in the sequence of Hebrew sound changes. The possibility that there are other such changes of which we are not aware is disturbing.

4.2. Question 3 (Change Favored by a Final Resonant)

The relationship between sonority and epenthesis has long been recognized by linguists. Sievers (1901a:294-5) and especially Jespersen (1913:191ff) have interesting things to say

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and yixbaə̄ — in fact there is evidence suggesting the opposite. It is quite likely, for instance, that in Jeremiah's dialect neither wayyiḥad nor lɔqaḥat had undergone apocope, cf. forms like wattizni-šɔ́m 'and she (!) committed fornication there' (Jeremiah 3:6) and ʔal-tɛ̄mḥi (paroxytone!) 'do not erase (masculine!)' (18:23), for the former, and ketivs like lmdty (2:33), qrʔty (3:4), šmᶜty (4:19), hlkty (31:20), hrbyty (46:11) and probably also forms like šɔvarti (2:20) and nittaqti (loc. cit.), for the latter. Forms like šɔmā́r and yixbā́ə̄, on the other hand, show no sign of having preserved their final vowels in Jeremiah's dialect or any other for that matter. Thus, Blau's theory about the date of apocope in verbs should be restricted to the verb forms from which his evidence is drawn, evidence which, incidentally, can be explained on the basis of accent rather than syllable structure (cf. Brockelmann 1903:9fn1 and Nyberg 1952:§15a,b).

[21]This solution presupposes that spirantization was no longer productive at the time of the Massoretes, but as Blau has pointed out (private communication):

"In Aramaic, at any rate, spirantization was a living feature even in Saadiah's time (v. his commentary to Sefer Yeṣirah). It is to be assumed that the same was the case in Hebrew, so decisively influenced by Aramaic."

This is a serious objection which I am unable to dispose of in a convincing manner. It is true that it is a long way from the Aramaic vernacular of women in 10th-century Baghdad to the Biblical Hebrew reading tradition of the Massoretes in 8th-century Tiberias, but the gap is narrowed considerably by the existence of segolated but unspirantized verbs in Biblical Aramaic (hištɔxáḥat 'you have been found', Daniel 5:27) and Babylonian Hebrew (yáḥad 'let it rejoice?; let it be united?', Job 3:6 in Yeivin 1973a:54,130). I have decided to retain Harris' explanation because I fail to see any viable alternative to it: the various analogies that might be proposed to account for the unspirantized t in lɔqā́ḥat, šɔláḥat, pɔθáḥat, etc. are not capable of accounting for the unspirantized d in wayyiḥad.

on this subject, but, for our purposes, the most useful discussion is that of Bloomfield ([1933]1965:384):

> "When a relatively sonorous phoneme is non-syllabic, it often acquires syllabic function;[22] this change is known by the Sanskrit name of *samprasarana*. Thus, in sub-standard English, *elm* [elm] has changed to ['elɱ]. This is often followed by another change, known as *anaptyxis*, the rise of a vowel beside the sonant, which becomes non-syllabic. Primitive Indo-European *[agros] 'field' gives pre-Latin *[agr]; in this the [r] must have become syllabic, and then an anaptyctic vowel must have arisen, for in the historical Latin form *ager* ['ager] the *e* represents a fully formed vowel. Similarly, Primitive Germanic forms like *['akraz] 'field', *['foglaz] 'bird', *['tajknan] 'sign', *['majθmaz] 'precious object' lost their unstressed vowels in all the old Germanic dialects. The Gothic forms [akrs, fugls, tajkn, majθms] may have been monosyllabic or may have had syllabic sonants; anaptyxis has taken place in the Old English forms ['ɛker, 'fugol, 'ta:ken, 'ma:ðom], though even here spellings like *fugl* are not uncommon."

That the same relationship between epenthesis and sonority may have existed in Arabic (in the pausal forms of nouns on the pattern CVCCun) was pointed out already by Brockelmann (1908:I, 209) and Schaade (1911:58), and Harris (1936:34) was able to demonstrate its existence in Punic as well:

> "Beginning with Punic there are traces of anaptyxis in doubly closed syllables, similar to the occurrence of anaptyctic vowels to simplify the pronunciation of the Hebrew segolates. As would be expected, the words in which this occurs are those in which the last consonant is more sonorous than the preceding, thus making a group which is normally not a single syllable at all and which is very difficult to pronounce. For קבר *qabr* 'grave,' there is the Punic variant קבער, with an anaptyctic vowel. In Neo-Punic, *nidr* 'vow' is often written נדער; *sikr* 'memory', סכער; and *ʿasr* 'ten', עסער."

, Evidence that the relationship may have also held in Hebrew was adduced by Speiser ([1926] 1967) in 1926. (Blake's unsuccessful attempt (1911:219) to prove that Tiberian Hebrew had syllabic resonants in word-final position need not detain us). Speiser observed (op.cit., 390-2) that in Hebrew nouns and verbs which end in a consonant cluster, i.e., forms which should have undergone epenthesis but didn't, the first consonant of the cluster is generally more sonorous than the second (although it is clear that other factors besides sonority are at work in these forms[23]). The forms in question are: *nerd* 'nard', *ʾard* 'Ard (pr.n.)', *yerd*

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[22]Cf. Bell 1970 for a detailed analysis of this change, based on data from a large group of languages.

[23]It is no accident that the epenthesis-resistant cluster in four of the forms is *rd* and that it is a voiceless sibilant (generally *s*) plus a voiceless stop (generally *t*) in five or six of the others. Since *r* and *d* are homorganic, epenthesis between them would produce a sequence close to the C_xVC_x sequence which many languages tend to eliminate (probably because of the delicate coordination of opposing muscles required to move an articulating organ back and forth quickly, cf. "tongue-twisters" like "Peter Piper picked a peck of pickled peppers") by metathesis (cf. the "2-2 Contraction" rule of Arabic (Hetzron 1974:6-7)), syncope (Bell 1970:12), or haplology. (Counter examples like *méred*, *zéred* (pr.n.) and *séred* (pr.n.) result from the momentum that Wang (1969:22) has dubbed the "snowball effect" and do not affect the validity of the argument.) A similar point is made by Sievers (1901a:295):

'have dominion', *yaṛd* 'give dominion', *qošṭ* 'truth', *košṭ*[24] 'putchuck', *yešṭ* 'turn aside', *yešt* 'drink', *wayyašq* 'gave to drink', *wayyevk* 'wept', *yaft* 'make wide', *wayyift*[25] 'was enticed', and the entire group *qotalt* (but cf. *wayyišb*[26] 'took captive'). From this evidence and some rather too carefully selected parallels in Arabic, Ethiopic, and Akkadian, Speiser deduced (ibid., 392-3) that:

> "When two consonants were left in the Semitic languages at the end of a word, there arose the need to develop a secondary vowel in the final syllable if the last consonant was more sonorous than the preceding one. For practical purposes it may be said that this was the case between a stop or sibilant and a following liquid or nasal. Thus arose forms like Arab. *mahal*, Hebrew **ʾaben > ʾeben*, Akk. *ṣalam*. That there was no phonetic need for the development of a segolate vowel if that order of consonants was reversed is proved by the fact that in modern Arabic there is no anaptyxis under such conditions, hence we get here forms like *qalb, milh*. The same is true of the short imperfect of the Hebrew verbs of the type *tertiae wy*, hence *yiben* occurs in that language alongside of *yaṛd* and *yešt*[27] .... And finally, in some languages the vowel was extended to all nouns capable of segolization, notably in Hebrew where *ʿebed* is now found by the side of *ʾozen, geber*."

> "Svarabhakti tritt um so leichter ein, je grössere Schwierigkeiten sich einer raschen Umsetzung der Articulationsstellung darbieten, d.h. je grösser die Articulationsdifferenz der Nachbarlaute ist. Zwischen nahezu homorganen Lauten tritt sie daher ausserst selten auf, so etwa zwischen *n + d, n + t*."

As for the failure of *qošṭ, košṭ, yešṭ, yešt, wayyaša*, and *wayyišb* (if pronounced [wayyišp]) to undergo epenthesis, it is of a piece with the irregular syncopation of vowels in the environments *šš_t* and *š_tt* in languages where syncope is normally restricted to (short) vowels in two-sided open syllables, e.g. P-S **ʾiššatu- > *ʾištu-, *šiššatu- > *šištu-* (Steiner 1975:2,5-6,8), Hebrew **šittayim > štayim*, Syriac *šittā > štā*, and perhaps also Akkadian *ina šattaqad* > Aramaic *ʾeštəqaš* (Speiser [1926]1967:377fn16). Both of these phenomena, like the metathesis which produced Biblical Aramaic *ʾištiw* 'they drank' (< **š štiw*), reflect a preference for uninterrupted sequences of voiceless sibilant (esp. *š*) followed by voiceless stop (esp. *t*), but the reasons for this preference are not entirely clear.

[24]A Mishnaic term attested in *ʿUqṣin* III.5. For the vocalization, cf. Mishna Codex Parma B.

[25]That *yaft* and *wayyift* already had fricative *f* (instead of earlier *p*) by the time verb epenthesis took place follows from the relative chronology proposed by Harris (cf. above, p. 9) on the basis of forms like *yiḥad*.

[26]This is probably an exception, but it is also possible that final *b* was devoiced in this word.

[27]Speiser appears to be less than candid when he contrasts *yaṛd* and *yešt* with forms like *wattēmen, yiven, yifen* (ibid., 391,393), leaving his readers to infer that the law of sonority is a synchronic fact within the realm of final-*y* apocopated imperfects in Hebrew. Actually, there are numerous counter-examples to the law of sonority even in this limited sub-class (e.g. *tireš, yiren, wayyimeš, tēreš, wayyireš, wattēmeš, teren* and perhaps also *wayyišb*, cf. fn. 26). It is true that all or most of the counter-examples exhibit "over-epenthesis" with respect to the law of sonority rather than "under-epenthesis," but this is a much weaker statement than the one which is implicit in Speiser's discussion.

Speiser's hypothesis is reasonable, even though it goes far beyond the evidence adduced by him. Moreover, it provides a simple answer to question 3, for if segolates ending in a resonant were the first to undergo epenthesis, then it is possible to assume that they were the only ones (aside from segolates with a medial laryngal like * baᶜd* and *qaḥt*) which underwent epenthesis early enough to be affected by stress-shift. Accordingly we must modify the diagrams on pp. 9 and 10 as follows:

CONSTRUCT APOCOPE

FEEDS   *FEEDS*

PHILIPPI'S LAW   FINAL RESONANT &
                                                  MEDIAL LARYNGAL
                                                  CONSTRUCT EPENTHESIS

COUNTER FED BY | FEEDS

ABSOLUTE APOCOPE   STRESS-SHIFT

FEEDS   *COUNTER FED BY*

ABSOLUTE EPENTHESIS
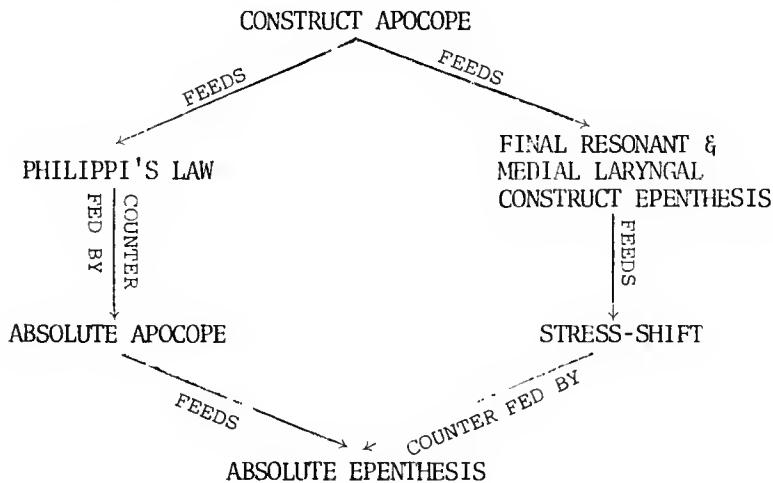
Figure 5

| FINAL RESONANT &
MEDIAL LARYNGAL
CONSTRUCT EPENTHESIS	qə̌ḥāθ
-- STRESS-SHIFT	
ABSOLUTE EPENTHESIS	(ləɔ)qaḥaθ
-- SPIRANTIZATION	
VERB EPENTHESIS	ləɔqaḥat

Figure 6

Further evidence for Speiser's hypothesis comes from a small group of segolate toponyms, alluded to above (§3.1), which underwent epenthesis early enough to be affected by stress-shift: *šɛ̌xɛ́m, gəvấl,* and, less certainly, *səɔ̃ôm.* That these toponyms are indeed segolates is perfectly clear (except in the case of *səɔ̃ôm*) from allomorphs which occur with locative and gentilic suffixes (*šɛ́xmɔ(h), givlî,* and *səɔ̃mîθ*[28]), from transcriptions in the Amarna letters

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[28]This form is listed, without reference, by Yeivin 1973a:145. The source is apparently *Sifra,* Codex Assemani 66, p. 52, where examples appear in lines 8 and 9. The form is spelled with a *šüǔb* after the *ɔ̃,* but *šüǔb* in this manuscript, as in all or most other

(Šakmi, Gubla) and other ancient documents (cf. Koehler, Baumgartner, et. al. 1967, s.v. gə̆vå̆l),
and, to a lesser degree, from variant spellings in the Isaiah Scroll (śwdm) and the Genesis
Apocryphon (śwdm, śwdwm) (cf. Kutscher 1959:83-4). Here again we find that all of the forms
in question end in a resonant.

It remains to be said that, while it is clear that śɔ̆xɛ̆m, gə̆vå̆l, and possibly śə̆ðɔ̆m underwent
epenthesis earlier than absolute-state COMMON nouns ending in a resonant, it is not all clear
why this should be so. Could it be that proper nouns were used in "pausal" form (i.e. with-
out case-endings) in ancient Hebrew as Brockelmann (1903:5fn1) suggested? Or does the excep-
tional behavior of these place-names merely indicate that they were borrowed from the dialects
of the cities to which they refer (Malone 1971:56, Bauer and Leander [1922]1965:580)? Further
research is clearly called for.

A more difficult problem is posed by the existence of COMMON nouns stressed on their epenthetic
vowel in the absolute state as well as the construct. It is well-known that nouns ending in
y generally fall into this category. Since y is even more sonorous than the liquids and nasals,
it is not surprising that these nouns should be more susceptible to early epenthesis and, con-
sequently, to stress-shift. The only question is whether the oxytone allomorphs spread to the
absolute state by analogy[29] or whether epenthesis preceded apocope in final-y nouns.[30]

Nouns with medial ˀ are also regularly stressed on their epenthetic vowel in the absolute state,
and much of what has been said about final-y nouns applies with slight modification to this
class of nouns as well. We might also note that the relationship between the medial-ˀ nouns
and the other medial-laryngal nouns is very similar to the relationship between the final-y
nouns and nouns ending in a liquid or nasal.

The most difficult problem of all is posed by nouns which are stressed on their epenthetic
vowel in the absolute state, but yet do not fall into either of the above classes. Some of
these (gə̆vå̆r,[31] śə̆lɔ̆w) end in a liquid or semi-vowel, but some (də̆vå̆š, śə̆vɔ̆x, śə̆vå̆x?) do not.

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manuscripts with Early Babylonian vocalization (Yeivin 1973a:70), often stands for ety-
mological and phonetic ϕ. The vocalization with o (although it is possible that in line
9 this has been corrected to u) is puzzling (one would normally expect u in the Babylo-
nian system), but not entirely unparalleled; cf. Yeivin 1973a:63(several examples),202
(ˀoxle(y) 'foods'), 204 (torpo(h)). Incidentally, the form we are discussing is not a
hapax. There are many examples of graphemic ⟨swdmy⟩ scattered throughout Rabbinic
literature (Kutscher 1959:84,Sokoloff [1969]1972:295) which are clearly to be read śuðmi
(Kutscher loc. cit.) or śoðmi. Finally, we should note that if we accept Kutscher's
derivation (loc. cit.) of śwdmy from an original *śudumiyyu, the Sifra's vocalization
becomes less difficult to explain, although one might still have expected śuðmiθ or
śə̆ðɔ̆miθ. It may, therefore, be necessary, in the end, to abandon our classification
of the name śə̆ðɔ̆m as a segolate.

[29] Analogy was more likely with final-y nouns than with, e.g., final-*h* nouns, because more
final-y construct forms had undergone epenthesis.

[30] In the latter case, we would posit *gadyu > gadiu > gadiyu*. Changes of this type (which
increase the number of syllables in a word) are discussed by Jespersen (1913:193,198-9).
This solution would allow us to account for forms like śə̆viyyɔ̆ (Song 4:5,7:4) and
gə̆ðiyyoθåyix (Song 1:8) without invoking analogy.

[31] This form, which occurs in the phrase gə̆vå̆r tɔmim (Psalms 18:26) is often said to be a
construct form. This view presupposes that tɔmim can be a noun as well as an adjective,
an assumption which is highly questionable. Not surprisingly the Hexapla vocalizes γαβρ
θαμιμ.

These forms are anomalous in other ways as well: 1) pre-tonic vowels, even *a*,[32] have been reduced (Malone 1971:56fn40), even though the tonic vowels are etymologically short,[33] and 2) stressed *a* has not been replaced by ɔ (except before *w*[34]) even though these are nominal forms. For these forms, the assumption of inter-dialectal borrowing (Malone 1971:56, Bauer and Leander [1922]1965:580) would seem to be the only solution.

4.3. Question 4b (the Vowel *a*)

If we accept Malone's assumption (1971:46) that the original epenthetic vowel was uniformly *ɛ*, we must explain how this vowel was lowered to *a* in *ḥăδăr*, etc. Two possibilities come to mind. On the one hand, we might attribute the lowering of *ɛ* to the presence of *ħ* (and, of course, the laryngals; cf. Malone's rule (hH), loc. cit.), especially if it took place while the *ɛ* was still unstressed, cf. **wayyăśiħ* 'he removed' > *wayyɔ́śaħ*, and **wayyăˁiħ* 'he wakened' > *wayyɔ́ˁaħ*. If, on the other hand, the lowering of *ɛ* took place after the stress had shifted to it, Philippi's law would seem to provide the answer.[35]

Neither of these solutions is without its difficulties. The former solution is incapable of explaining why the epenthetic vowel of *śɔ́γăn* was lowered, while the latter solution fails to explain why the epenthetic vowel of *ħăvɛ̄l* was NOT lowered. Nevertheless, the latter solution seems preferable, since there are other exceptions to Philippi's law which must be accounted for in any case: **ˀăvɛ̄l*[36] (note the striking phonetic similarity to *ħăvɛ̄l*!), *ˁăqɛ̄v*, and *ħămɛ̄ś*. Rather than explain these forms away as analogical restorations (a strategy which, in any event, will not work with *ħăvɛ̄l*, since the corresponding absolute form is not **ħɔvɛ̄l* but *ħɛ́vɛl*), we propose a solution based on the possibility that Philippi's law and stress-shift partially overlapped in time.

A glance at figure 5 reveals that Philippi's law and stress-shift at least fall within the same general time-period (after construct apocope and before absolute epenthesis[37]), and that stress-shift could easily be the later of the two. At the same time, it is well-known that stress-shift feeds Philippi's law. We may therefore hypothesize that stress-shift began in time to supply SOME customers to Philippi's law, but that the latter became extinct before the former had worked its way through the entire class of bi-syllabic construct forms.[38]   In other words, it is possible that we have COUNTER-feeding here as well

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[32]Short *a*, the most sonorous of the short vowels, is generally immune to pre-tonic reduction in Biblical Hebrew. Final-*y* segolates like *gɔ́δi* (< **gadyu*) and *śɔ́vi* (< **śabyu*) are only apparent exceptions to this rule, since the original *a* of these forms was raised to ɛ by vowel harmony (cf. *kɛlyɔ́xɔ*, *tɛlyɔ́xɔ*, *pɛħyɔ́xɔ*, and *gέδi*, *kέli*, *pέħi*, and the proper nouns *ˀɛvyɔ́sɔ́f*, *ˀɛvyɔθɔ́ħ*) before being reduced.

[33]This last detail, unmentioned by Malone, is crucial, cf. fn. 15, above.

[34]The ɔ in *śɔ́lśw* may be a product of assimilation (*a* becoming rounded before *w*) at a late stage, rather than a product of stress-lengthening, cf. Blau 1967:63, and add *wayśɔ́św* (I Samuel 21:14) to the examples cited there.

[35]This solution was suggested to me by Norman Didia, a student of mine at Touro College.

[36]The form which is actually attested is *ˀavɛl-*, i.e. with ɛ and *maqqéf*; cf. p. 7 above for other examples of this alternation.

[37]I.e., the first stage of absolute epenthesis — affecting nouns ending in a resonant. This must have taken place very soon after absolute apocope. Accordingly, the time-period covered by this diagram is not so long as to invalidate the argument which we wish to base on it.

[38]Wang 1969, which I came across after writing these lines, argues, on theoretical grounds, that competition between (= the overlapping of) sound-changes is one of the causes of sound-change residue. The Hebrew data presented here certainly seem to support Wang's position.

as feeding³⁹ — and that *hăvêl*, **ʾăvêl*, *ᶜăqêv*, and *hămêš* are "products" of this counter-feeding. As such, *hăvêl* would no longer stand in the way of a solution to question 4b based on Philippi's law.⁴⁰

5. CONCLUSION

This investigation has shown that forms like *hăδăr* and *hăvêl* are relics of an early stage in the development of Hebrew. The occurrence of these forms in Tiberian Massoretic Hebrew is significant, firstly because some of them (e.g. *šδγăr*, *zδrăᶜ*, *nδtăᶜ*, *hasăr*) must have already disappeared from colloquial Hebrew by the Mishnaic period, and secondly, because a number of them have been replaced by their absolute-state co-allomorphs in the Babylonian tradition of Biblical Hebrew, e.g. *hăvăl* (Kahle [1902]1966:171-2), *šăγăr* (Yeivin 1973a:193), *šăvăᶜ ᶜšrh, šăvăᶜ mʾwt, tešăᶜ mʾwt* (ibid., 220).⁴¹ These facts must now be added to the growing body of scientific evidence (cf. esp. Kutscher 1959:23-52) indicating that the Tiberian vocalization is a faithful, even slavish, reproduction of a stubborn oral tradition which succeeded in preserving ancient forms even after they had disappeared from the Hebrew spoken by the bearers of that tradition.

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³⁹ This should not be confused with Koutsoudas, Sanders, and Noll's "feeding and counter-feeding" (1974:2), which is a symmetric relation between two NON-OVERLAPPING rules. What we are proposing is that PART of stress-shift precedes the extinction of Philippi's law (and thus feeds it) and part of stress-shift follows it (and thus counter-feeds it).

⁴⁰ Another solution to question 4b might be to reject Malone's assumption that there was only one epenthetic vowel and to assume that, in the earliest period, the epenthetic vowel took on the coloring of the stem vowel (as it does in some Arabic and Akkadian dialects), except when the word ended in a laryngal or *r*. This assumption is made plausible by the fact that *hăvêl* is apparently a *qitl* form (cf. *hevlô*, *hevlɛxɔ*), while *šδγăr* seems to go back to a *qatl* form (cf. Assyrian *šaknu*, from which it is borrowed). But construct epenthesis would still have to be ordered before stress-shift, and stress-shift before the extinction of Philippi's law, and, accordingly, the unlowered *e* of *hăvêl* would still be a problem. This "solution" is therefore illusory.

⁴¹ Cf. also *šδγăn* (abs.) for *šɛγɛn* in *Sifra*, Codex Assemani 66, p. 180. Older forms preserved by the Babylonian tradition are *bδᶜăδ* (Yeivin 1973a:198), *šδhăr* (ibid., 197), and *hsăr* (Yeivin 1973b:168).

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THE LOSS OF FINAL CONSONANTS IN

BABYLONIAN JEWISH ARAMAIC (BJA) *

by

Daniel Boyarin
The Jewish Theological Seminary of America, New York

The purpose of this paper is to explain the sporadic loss of
final continuants in certain BJA forms as owing to metanalysis
of forms in which the final consonant was assimilated to the
enclitic preposition [l-]. For example, */ɔmarleh/ ('he said
to him') developed to /ɔmalleh/, thence to /ɔmaleh/, thence
metanalyzed as /ɔma/ + /leh/, yielding the new verb form /ɔma/
('he said'). One necessary step in the above development is
neutralization of consonant length, implying
that such neutralization had already taken place in this dia-
lect. The case for this explanation is strengthened by compar-
ative and intra-dialectal evidence.

One of the striking features of Babylonian Jewish Aramaic (BJA) is that the final consonant
of certain verbal roots is often lost. This phenomenon is most common in the late or Geonic
dialect.[1] It is more restricted but still well attested in the normal Talmudic dialect, and
considerably less prevalent in the special (archaic?) dialect of certain tractates.

All of the consonants so affected are voiced continuants. They are /l/, /m/, /r/, /b/, and
/d/.[2] Examples have been gathered by Margolis and Epstein.[3] Among them are

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*I wish to thank Professors H.L. Ginsberg, Stephen J. Lieberman, and Joseph Malone for
their aid in formulating this paper. Also grateful acknowledgement is due the Maxwell
Abbell Foundation for Grants-in-Aid enabling me to pursue my studies. I have also dis-
cussed some points with my friends, Prof. Richard Steiner and Prof. Yona Sabar. Of
course, responsibility for all opinions expressed is my own.

[1] For a general introduction to BJA and catalogue of the sub-dialects see J.N. Epstein,
A Grammar of Babylonian Aramaic, (Hebrew), ed. E.Z. Melamed, (Jerusalem, 1960) pp. 13ff.

[2] In Hebrew and Aramaic /b/ and /d/ (as well as /t/, /k/, and /g/) were realized as contin-
uants post-vocalically. In BJA the continuant allophone of /b/ was almost definitely a
frictionless, voiced bilabial continuant [w] (cf. Malone, *Lešonenu* 37, p. 161).

[3] M. Margolis, *A Manual of the Aramaic Language of the Babylonian Talmud*, (München, 1910)
p. 10. Epstein, pp. 57ff. /n/ and /t/ may be added if we admit the examples in n. 23.
In this paper italics will be used for graphemic representation, // for phonemic, and
brackets for occasional phonetic. I am assuming only one front vowel phoneme for BJA,
an assumption which, while perhaps questionable, does not affect the argument of this paper.

ᵓzℓ   ('he went')   ∼   ᵓzᵓ /ᵓza/.

ᵓzyℓ ('going')   ∼   ᵓzy /ᵓāzi/.

šqℓ   ('he took')   ∼   šqᵓ /šqa/.

šqyℓ ('taking')   ∼   šqy /šāqi/.

ᵓyqbyℓ ('I will receive') ∼ ᵓyqby /ᵓiqabi/.

tyqwm   ('it will stand')   ∼ tyqw /tiqū/.

qᵓym   ('standing')   ∼ qᵓy   /qāj/.

qym   ('established')   ∼ qy   /qi/.

ᵓymr   ('I will say')   ∼ ᵓymᵓ /ᵓima/.

ᵓmr   ('he said')   ∼ ᵓmᵓ   /ᵓma/.

nšb   ('it blew')   ∼ nšᵓ   /nša/.

nyᶜbyd ('he will do')   ∼ nyby /nibi/.

Many more examples are found in the cited secondary literature, but these will suffice to illustrate the process. To my knowledge, Epstein makes no attempt to account for this phenomenon.[4]

In brief, my explanation is that in certain morpho-syntactic conditions these consonants were assimilated to the initial liquids of common affixes, and later, when long or geminate consonants were not phonemically distinguished from short/nongeminate, the resulting form was re-analyzed and, by analogy, the previous bound form substituted for the previous free form.[5]

Obvious examples of such assimilation are such forms as ᵓzynᵓ /āzina/, ('I am going') or šqyn /šāqina/ ('I am taking') from/ᵓāzilna/ and /šāqilna/ respectively.[6] It is easy to see why speakers of the dialect would analyze these forms as /ᵓāzi/ and /šāqi/ respectively plus /na/, just as /hāwena/ is /hāwe/ plus /na/.

However, the primary operative morpho-syntactic conditioning for the loss of final root consonants was apparently the preposition [1-] used enclitically. Evidence for the enclisis of the prepositions [1-] and [b-] is found quite clearly in the excellent Hamburg manuscript of B.T. Mas. Nezikin.[7] There we find ḥyybbh, written clearly as one word (3x!), ptrbh,[8]

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[4]Nor does anyone else of whom I am aware. But see T. Nöldeke, Mandäische Grammatik, (Halle, 1875) p. 50 and cf. below n. 20.

[5]Determination of sufficient conditions for this development awaits a full-scale phonological treatment of the language including a distinctive feature analysis. The author is presently engaged in the early stages of such a study.

[6]J.A. Montgomery, Aramaic Incantation Texts from Nippur, (Philadelphia, 1913) p. 143.

[7]The Babylonian Talmud Codex Hambourg (sic) 165, facs. (Jerusalem, 1969). For an excellent linguistic discussion of this manuscript see E.Y. Kutscher, "Research on Babylonian Aramaic Grammar" (in Hebrew), Lešonenu, 26 (1961) pp. 174-177.

[8]Both examples are on Baba Qamma 13b. For the orthography cp. Mandaic šᵓkybbh 'he sleeps in it' with b written twice. R. Macuch, Handbook of Classical and Modern Mandaic (Berlin, 1965) p. 12.

ʾybᶜyʾln.[9]   In the Columbia University manuscript of *Mas. Pesahim*, there is *maqšwlk*, (=
'they object to you').[10]   Furthermore, Epstein has recorded a form *pršnlhw*, (= 'we have
explained them').[11] Also in the important Spanish ms. of *Av. Zar.* published in facsimile by
S. Abramson[12] I have found *hwtly* and *tymwlyh* both clearly written.[13] The existence of such
enclitic forms ought not surprise us as such pronunciation is clearly indicated in Biblical
Aramaic by the Massoretes, through their use of the maqqef, /wyeʾmar-leh/.[14] An orthography
similar to the ones attested above for BJA is already found in the Gen. Apoc. 2, 9 *dkrlk*.[15]
In Mandaic, enclitic use of these prepositions is the rule, and therefore it is with some
degree of certainty that we may say it was in Babylonian Jewish Aramaic as well.

I wish to propose that the final consonant of the verb was assimilated to the /l/ of the en-
clitic preposition, and that by metanalysis the resulting bi-radical root morpheme came to
be used in positions even where free of the enclitic. I have found intra-dialectal evidence
for the assimilation hypothesized, in the forms /qillah/[16] and /mašgillih/.[17] It should be
pointed out that all verbs would function with the enclitic /l/ in BJA because it marks
direct object, and indirect object, as well as being used ingressively in such phrases as
/ʾzal lih/ ('he went').

Therefore the proposed development of these forms would be, for instance (1) /ʾimar/ ('I
will say'), > (2) /*ʾimarlik/ ('I will say to you'), > (3) /ʾimalik/[18] > (4) /ʾima/ ('I
will say'), the most common of these forms. All stages of the process are attested in the
above-mentioned form from *Hal. Pes.* where we have also attested *qy ly*.[19] Therefore /qim li/
> /*qimli/ > /qil li/, and finally with simplification of the doubling and metanalysis >
/qi li/. Again the transfer of /qi/ and related forms to other environments is easy to ima-
gine and indeed occurs.[20]

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[9]*Baba Qamma* 20a.

[10]*Pesachim* 140b. This excellent manuscript has never been published. See Kutscher p. 177.

[11]J.N. Epstein, *Gaonische Kommentar zur Ordnung Tohorot*, p. 150.

[12]*Tractate Abodah Zarah of the Babylonian Talmud*, ed. S. Abramson (New York, 1957).

[13]p. 96.

[14]Daniel 4, 32. See F. Rosenthal, A *Grammar of Biblical Aramaic* (Wiesbaden, 1968 p. 18 for
other examples.

[15]N. Avigad and Y. Yadin, A *Genesis Apocryphon* (Jerusalem, 1958) for photograph. n.p.

[16]For /qim lah/. /kiwan dʾtta la qil lah bšum la samka daᶜtah/ ('Since a woman is not expert
in evaluation she does not depend on it.') *Halakot Pesukot*, facs. (Jerusalem: 1971) p. 106.
See Kutscher pp. 173-174. The form *gym l*- in this meaning is of course well attested in
Talmudic literature.

[17]From *mšgyr lh* ('he sends to him'). L. Ginzberg, *Geonica* (New York: repr. 1968) vol. II,
p. 104.

[18]Well attested, e.g. B.B. Lewin, *Otzar Hageonim*, (Jerusalem: 1931) Vol. I, p. 61.

[19]P. 96. /hani mili hika dla qi li bgawih mar qi li bgawih/ (= That applies when he is
not well known to me. Master is well known to me.)

[20]Although, ironically, not attested with this particular verb.

It must be admitted that it is not completely impossible to imagine a purely phonological
solution to this development, i.e. a vocalic realization of these consonants in final posi-
tion or some other type of simple apocope.[21] The following considerations, however, render
such an explanation considerably less likely. 1) The doubled /l/ in /qillah/ and /mašgillih/
militates against such an assumption. 2) The somewhat skewed distribution of affected
phonemes supports the assimilation explanation. While all of the affected consonants could
easily assimilate to /l/ (the liquids for obvious reasons, [ṯ] and [ḏ] because of their
similar point of articulation to /l/, and /b/ because of its phonetic realization as [w])[22],
it is hard to imagine why general apocopation of voiced continuants would not affect all
equally. But see n. 5. 3) There are partially related phenomena in closely related dialects
which cannot, by any means, be interpreted in such manner:

> "In Modern Mandaic the final *r* of AMR 'to say' regularly assimilates to the
> encl. (1). Such assimilated forms are then often written phonetically
> *ᶜmala* (Morg. 273) *emalla* 'he said to her.'"[23]

Mandaic is a dialect intimately related with Babylonian Jewish Aramaic and therefore it seems
to me that *ʾmalla* in Mand. cannot be separated from *ʾmalih* ('he said to him') etc. in BJA.
Since in Mandaic there is no secondary root AMA, we must interpret this form as assimilation,
as of course, the pronounced doubling indicates as well. It follows that the BJA forms are
assimilated as well (i.e. and not to be explained as mere scribal survivals of /ʾma/ plus
enclitic /lih/, preserved orthographically because of the unusual shortness of the verb
form.) Mandaic, then, represents an earlier stage of the process than does BJA.[24]

In Mandaic, apparently, there was never any back-formation from these assimilated forms, per-
haps because of the retention of gemination.[25] In fact, the only exact Aramaic parallel I
know of to the process hypothesized here is in neo-Aramaic, where in Garbell's formulation,
"stem final *m* in the root *q-j/0-m* is replaced by zero (...); further, the objective suf-
fixes of the 2nd p. sg. m. and f. are optionally (but more frequently, except after the neg-
ative particle) affixed to the stem: *qu* (⊰ *qúlox* m. *qúlax* f.), pl. *qúmun* 'rise' (...) the
stem of *0-z-l* is suppleted by *si-* and takes the suffixes described for *q-j/0-m; si* (*√silox*
m., *sílax* f.), pl. *símun* 'go'"[26] It seems most likely that the suppletion in precisely

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[21]Nöldeke, p. 50 does refer to "Abfall" of /m/, and does adduce some seemingly clear cases
of it. However, this does not damage our argument appreciably.

[22]Cf. n. 2. cp. Neo-Syriac /hal/ 'give!' < /hawl/ < /hab+l/. Nöldeke, *Grammatik der Neu-
syrischen Sprache*, (Leipzig, 1868) p. 256.

[23]Macuch, p. 52.

[24]"It does not seem impossible to me that we may parallel this relationship with another. In
Mandaic the /t/ of perfect 1st and 3rd feminine is preserved except before the enclitics.
(Nöldeke, p. 225). Joseph Malone, in a review of Macuch's book (*Language* Vol. 45, 1969,
pp. 197-198) has already demonstrated that *both* forms are to be explained by assimilation.
Therefore, it seems likely that the Babylonian Jewish forms, generally without *t*, should
be explained as analogical extension of the forms with enclitics. The same may be true
for forms like /amrittu/ ('you say'), which again in Mandaic only exist before enclitics,
otherwise /amrittun/ is found. (Nöldeke, p. 233). Phrases like /ʾatun ʾamritu/ in BJA
point in this direction.

[25]For rare exceptions see Macuch p. 37.(cp. Malone *op. cit.* p. 200).

[26]Irene Garbell, *The Jewish Neo-Aramaic Dialect of Persian Azerbaijan*, (The Hague, 1965),
p. 71. The /m/ in /símun/ is apparently the result of another analogy viz. /qúmun/ ana-
lyzed by speakers as /qu/ and /mun/ in spite of the fact that the /m/ is original in the
root. (Prof. Y. Sabar, oral communication, 1974).

these two roots and no others is a result of metanalysis of the enclitic forms.[27]

Finally, it should be noted that there are other assimilation-elision phenomena in Aramaic, such as /nezzun/ < /nezlun/ in Syriac. Such phenomena occur in Babylonian Jewish Aramaic as well in nouns, such as /qola/ < /qolra/, /zuta/ < /zutra/. These are always post-consonantal, pre-vocalic, and are therefore irrelevant here as can be shown from their general occurence in Aramaic,[28] in contrast, of course, to the phenomenon discussed here.

In brief conclusion, the loss of certain root final consonants is to be understood as the result of a syntactically conditioned phonological change, extended beyond its original environments by analogy.

The implications of this analysis, if correct, are two-fold: philological and linguistic. As to the first, Babylonian Jewish Aramaic cannot be used as support for apocope or vocal realization of /r/ in final position. Therefore, for example, it cannot support the interpretation of ꜣlᶜz (= 'the name' /ꜣelᶜazar/), in the recently published Gibᶜat Mibtar inscription as /ꜣelᶜaza/.[29]

As to the second, we would have to conclude that gemination must already have been non-phonemic in this dialect in order to allow such metanalysis.[30]

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[27] The phenomenon would perhaps be more widespread in the dialect were it not for the fact that the /rl/ combination became /rr/ > /r/ in this dialect (Garbell, p. 70) therefore /mirox/ 'you said' not /*milox/. The metanalysis was therefore prevented.

[28] C. Brockelmann, *Grundriss der Vergleichenden Grammatik der Semitischen Sprache*, (Berlin, 1908) Ip. 159.

[29] E.S. Rosenthal, "The Inscription from Gibᶜat Mibtar," *Peraqim* 2, (Jerusalem: 1975), p. 343, n. 31. I do not wish, of course, to deny the correctness of the interpretation, only to question the support from BJA.

[30] Cp. Syriac /ntl/, certainly from *ntn+l* >*ntl+l*, where when false division takes place the /l/ goes with the root because of the gemination.

THE POSITION OF THE DECLINED kl IN SYRIAC

by

Iddo Avinery, Bar-Ilan University.

The position of some types of noun modifiers in Aramaic and Syriac does not always correspond to strict rules. The main types of noun modifiers of this kind are: the demonstrative pronouns, the numerals and *kl*. We shall try to prove that so far as concerns the third type, *kl*, some rules can be established.

Nöldeke writes that "Überaus oft hat ein Substantiv *kl* mit seinen Pronominalsuffix in Apposition neben sich, voroder nachgesetzt."[1] An examination of the Peshitta to the Pentateuch shows that *kl* always precedes the nucleus, except for the following cases:

(a) When the nucleus is a genitive construction, in which the second member is also a genitive construction or a relative clause. Examples: Lev. 4:8 *wtrbᵓ klh dtwrᵓ dhṯhᵓ* 'all the fat of the bullock' (but at the end of the same verse *wklh trbᵓ dᶜl gwyᵓ* 'and all the fat inwards'); Gen. 3:2 *dmn pᵓry ᵓylnᵓ dbprdysᵓ klhwn nᵓkwl* 'we may eat of all the fruits of the trees of that are in the garden' (but Gen. 2:16 *klhwn ᵓylnᵓ dbprdysᵓ* 'all the trees that are in the garden'). As was already mentioned, when the nucleus is a genitive construction composed of only two members, it will be preceded by *kl*. An exception is Gen. 41:36 *dnhwyn bᵓrᶜᵓ klh dmṣryn* 'which will be in all the land of Egypt.' This is not a translation of the Hebrew, since *kl* is not found in this verse. But *kl ᵓrṣ mṣrym* is always given as *klh ᵓrᶜᵓ dmṣryn* (e.g. ibid. ibid.:29, 54, 55). It may be suggested that where the translator had not before him the Hebrew *kl*, he felt himself free to deviate from the Hebrew order of words (see, for example, Num. 20:11). It seems therefore that the position of *kl* is influenced by Hebrew, at least as far as the examples where the nucleus is composed of one word only are concerned.

(b) When the nucleus is composed of a substantive and the demonstrative pronoun (e.g. *hnᵓ*).

Four constructions are here possible, of the type: (1) *hnᵓ klh ywmᵓ*; (2) *ywmᵓ hnᵓ klh*; (3) *klh ywmᵓ hnᵓ*; (4) *klh hnᵓ ywmᵓ*. Constructions of the type *ywmᵓ klh hnᵓ* or *hnᵓ ywmᵓ klh* were not found.

Some examples: (1) Gen. 31:1 *qnᵓ hnᵓ klh qnynᵓ* 'he got all this wealth (according to the Ambrosian codex[2]; Lee[3], as well as the Urmi and Mosul editions, read *hnᵓ qnynᵓ klh*); Lev. 18:26 *hlyn klhwn hṯhᵓ* 'all these sins'; Num. 5:30 *hnᵓ klh nmwsᵓ* 'all this law'. (2) Gen. 33:8 *ᵓymkᵓ lk mṣrytᵓ hdᵓ klh* 'from where do you have all this camp' (again the Ambrosian codex has been checked. Lee, Urmi and Mosul read *hdᵓ klh mṣrytᵓ*); Ex. 11:8 *ᶜmᵓ hnᵓ klh* 'all these people'. (3) Ex. 10:13 *klh ᵓymmᵓ hw* 'all that day'; Ex. 11:8 *klhwn ᶜbdyk hlyn* 'all these your servants'

[1] T. Nöldeke, *Kurzgefasste syrische Grammatik*, 2d. ed. (Daramstadt, 1969), §218.

[2] *Translatio syra Pescitto ... ex codice Ambrosiano* (Milan, 1476).

[3] S. Lee and G.E. Barnes, *Pentateuchus syriace* (London, 1914).

(in group (2) above we find a different construction, which might be explained as a variety);[4]
Ex. 19:7 *klhwn ptgm³ hlyn* 'all these words' (compare also Ex. 24:8). (4) Examples of this type
are to be found only in Deuteronomy: 3:21 *lkwlhyn hlyn mlkwt³* 'to all these kingdoms' (Lee,
Urmi and Mosul read *lhlyn klhyn mlkwt³*); 6:25 (and passim) *klh hn³ pwqdn³* 'all this commandment'
(Lee, Urmi and Mosul have *pwqdn³ hn³ klh*).

Examples of the group (2) are quite rare; therefore we may still maintain that *kl* precedes
the nucleus.

(c) When the nucleus is the demonstrative pronoun.

It seems that under the influence of the *hn³ klh ywm³* type, *kl* may follow the demonstrative
pronoun even when it is the nucleus: Lev. 10:19 *wmtyny hlyn klhyn* 'and all these have befallen
me'; Lev. 18:24, 24 *hlyn klhyn* 'all these'; Gen. 41:39 *hd³ klh* 'all this' (but here, too, usually
kl precedes, e.g. Gen. 10:29, 25:4).

(d) Except for what was mentioned in the above three groups only very few exceptions can be
found where *kl* follows: Lev. 8:21 *w³sqh mwš³ ldkr³ klh* 'and Moses burnt the whole ram'; Num.
14:21 *w³tmly šwbḥnh dmry³ b³rc³ kwlh* 'and all the earth shall be filled with the glory of the
Lord.'[5]

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[4]Cf. the writer's article, "Problèmes de variation dans la traduction de la Peschitta sur
le Pentateuque," *Semitica* 25 (1975).

[5]These notes may be considered as a contribution to the general problem of noun modifiers.
Takamitsu Muraoka, "Remarks on the Syntax of some types of noun modifiers in Syriac"
(*JNES* 1972,3), treats the problem of the numerals (and also adjectives + demonstrative
pronoun) modifying substantives. See also my ariticle "The position of the demonstrative
pronoun in Syriac" (*JNES* 1975,2).

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